SCIENCE

VOL. 77 — FRIDAY, APRIL 7, 1933 — No. 1997

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SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS

New York City: Grand Central Terminal
Lancaster, Pa. Garrison, N. Y.

Annual Subscription, $6.00 Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

TENDENCIES IN THE LOGIC OF MATHEMATICS[1]

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1. INTRODUCTION

I APPROACH this subject which I have announced for to-day, "Tendencies in the Logic of Mathematics," with some trepidation, with many misgivings, and, I trust, with due humility. To many, the logic of mathematics is old, fixed, immutable. To shake faith in it, even to express a doubt concerning it, is to these nothing short of heresy. As Professor C. I. Lewis, of Harvard University, says in the October issue of *The Monist:* "From Aristotle down, the laws of logic have been regarded as fixed and archetypal; and as such that they admit of no conceivable alternatives. Often they have been attributed to the structure of the Universe or to the nature of human reason; and in general they have been regarded as providing an Archimedean fixed point in the realm of thought."

[1] Address of the retiring chairman and vice-president of Section A—Mathematics, American Association for the Advancement of Science, Atlantic City, December 28, 1932.

Yet long ago, as I shall indicate, doubts appeared; and within my own lifetime definite and undeniable difficulties have arisen which leave no room for a complacent acceptance of the logic of the ancients in unchanged form. I shall attempt to-day to state some of the difficulties and some of the proposed remedies. We shall see that there is to-day, if not universal agreement on the details of new systems, at least essential agreement that fundamental changes are necessary. As Lewis remarks in the article quoted: "There are no laws of logic which can be attributed to the universe or to human reason in the traditional form." Acceptance of such doctrine, however, like acceptance of other revolutionary changes in the history of human thought, of which the most recent is the theory of relativity, comes slowly; always men's minds revert to older ways of thought, always the interpretations placed upon the newer statements may be mistaken, always there is need for slow and detailed statement

of the essential grounds on which the need for changes rests.

If, then, I seem to some of you present, who know full well all—or well-nigh all—that I shall say, to go too slowly over the familiar grounds, I beg you to bear with me, since there is real need for patient and repeated statement of these things. I address myself to-day essentially to those to whom the reasons for change are not thoroughly known. To them, and to others who may later read these lines, I wish to make clear in not too lengthy form the larger outlines, with some details.

I am saved the necessity of a long introduction, recounting the slow growth of mathematical rigor, by the fact of easy access by all of you to two reports essentially on the same field: the Gibbs Lecture, by Professor James Pierpont, presented at the Nashville meeting of these organizations in 1927, and the invited address by Professor Arnold Dresden, presented at the same meeting, both of which were printed in Volume 34 (1928) of the *Bulletin of the American Mathematical Society*.

Pierpont gave a general résumé, going back at least to the beginnings of the Calculus, and coming down the centuries from Newton and Leibnitz through Euler and Lagrange and Cauchy to the days of Weierstrass and Kronecker and Cantor and Poincaré, and thence to our own generation and the present decade, with sketches of the three chief movements of this period, which are associated in men's minds most vividly with the names of Whitehead and Russell, Hilbert, and Brouwer, respectively. These remain the three chief directions of research in this field, and it is to them that I shall give particular attention in what follows. By name, these three schools of thought, or methods of procedure, are most often called the logistic, the postulational or formalistic, and the intuitional schools or methods, respectively. While my own sympathies will doubtless appear, I will attempt to give an impartial account of the progress in these fields, in grand outlines, and their present states.

Dresden, in his address, states what appear to him to be the underlying philosophical aspects. Both here, and in a former report published in Volume 30 (1924) of the *Bulletin*, he gives special attention to the work of Brouwer and to its underlying philosophy, but he does so with great impartiality, and his views on the doctrines which underlie mathematics and mathematical logic are still worthy of careful study. These two reports by Dresden were preceded by a translation by him of an article by Brouwer, published in the *Bulletin* in 1913. The two papers in 1913 and 1924 deal primarily with Brouwer's work; with the present article, they span two decades, and it may be of interest to observe by a comparison with them the slow but certain progress of the principal ideas during these two decades.

2. Introduction of Infinities

In a large sense, though with certain notable exceptions, such as the controversies over the parallel postulate, the origin of logical difficulties appears to lie in the introduction of infinities into mathematics. Even in the case of the parallel postulate, the rôle of infinity is clearly apparent, and might be held to be the paramount difficulty. With the advent of the Calculus, at any rate, with its troublesome concepts of infinitesimal and of limit, the effect of infinities upon mathematics and mathematical reasoning becomes striking and undebatable. It is with these difficulties that Pierpont begins in his address.

That essentially the same difficulties had arisen in the old Greek paradoxes is now clear to us; but such paradoxes were regarded mainly as philosophical playthings, and the mathematical controversies in the same field awaited the important development of the Calculus. At one other point, however, an objection had been raised before that time; and it is so important for our more recent studies that I wish to emphasize it, particularly because it is not mentioned in the addresses quoted above. Isaac Barrow, who was one of Newton's teachers, and one to whom Newton owed much, had objected, before the Calculus controversy, to the development of the theory of proportion in the Fifth Book of Euclid. He pointed out that Euclid's argument, and in particular his definition of the sense of inequalities for what we should now call irrational numbers, requires, for a determinate answer, the use of an infinite number of steps. This objection, which Barrow stated in clear language, makes him the forerunner of such modern scholars as Kronecker, Poincaré, Brouwer, Weyl and E. T. Bell, all of whom have reiterated this objection. In it, the notion of infinity enters in mathematical reasoning again, in a very different, and in a very disturbing fashion.

The position of Kronecker is stated in some detail by Pierpont. In his *Festschrift*,[2] Kronecker attacked particularly the definition of reducibility of a polynomial, which is ordinarily stated by saying that a polynomial is reducible, or not, according as it has, or has not, a rational factor. Without means for decision, and faced with a possible infinite number of trials, Kronecker declares such a definition not only unusable, but meaningless. For the same reasons, he attacked bitterly the whole theory of irrational numbers, and the entire structure of the Weierstrass theory of functions. To this day, these objections

[2] *Zeitschr. für Math.*, Vol. 92. 1882.

stand. They have been voiced again and again, most strenuously, perhaps, by Brouwer, which may be a misfortune. To many other great mathematicians, however, including Poincaré and Weyl, objections of this character, involving both definitions and existence proofs, have seemed weighty and soberly ominous. It is indeed surprising that so little heed has been paid to the reasonable warning given time and again by men of the first rank, from Barrow to Poincaré; and it must be admitted that every such definition, every such existence proof, which does not contain in itself an explicit means for setting up the concept discussed in a finite number of steps, is open to question, and can not be said to be established with logical rigor.

That insistence upon such a point would destroy much of mathematical literature, as has been urged by such equally great figures as Weierstrass and Hilbert, seems to me to be as much beside the point as is that argument for immortality which depends upon our displeasure at mortality. If there be no better argument, we may as well begin at once some effort toward reconstructing those parts of mathematics that depend on such debatable procedures.

In one instance, this has already happened. In 1897, Burali-Forti[3] published his now famous paradox on transfinite numbers. As a result, that theory is now quite re-formed, and parts of it are no longer used, while some theorems, such as the theorem that every closed set consists of a countable set together with a perfect set, once proved by means of that theory, is now commonly proved otherwise. Possibly due to this very paradox, and surely due to some similar considerations, Poincaré showed a most interesting change of attitude—a volte-face, indeed—from the Paris Congress in 1900, where he said, "One may say to-day that absolute rigor has been attained," to the Rome Congress in 1908, where he said, "Later generations will regard the Mengenlehre as a disease from which one has recovered." His other writings from that time on betray his lack of faith in that rigor which he had called "final" in 1900.

Such objections are climaxed and most vociferously proclaimed by Brouwer, as I have stated above, but he is by no means alone.[4] These objections, however, should be differentiated from what is the central part of Brouwer's thesis, the so-called intuitionism. It is by no means necessary to become an adept of intuitionism in order to adopt the objections of Kronecker and Poincaré, which Brouwer also advocates. Nor is Brouwer's objection to the Law of the Excluded Middle an integral part of intuitionism. One may, and, as I shall point out, one now must, admit that the Law of the Excluded Middle is not a necessary part of logic, whether one is an intuitionist or not. All these objections are advocated strongly—perhaps too strongly—by Brouwer, and his adherents as well as he will justly claim them as an integral part of his whole thinking and of his works. This I will not deny. What I wish to emphasize is that neither the objection to an infinite number of steps in an argument or definition or existence theorem, nor the objection to the Law of the Excluded Middle as a fixed principle, is so tied to intuitionism that one must accept it in order to accept these others. I believe that the general impression that these ideas can not be accepted without acceptance of all that is associated with Brouwer's name is one of the reasons why the mathematical world has so long delayed acceptance of them, and action based upon them.

3. Brouwer's Position. Excluded Middle

In what precedes, I have dwelt upon one objection voiced by Brouwer, but also by many great scholars before him. His objection to the Law of the Excluded Middle is, as I have remarked, also independent of the method or school called intuitionism. Leaving that method for later discussion, I wish to call attention now to his position, and the position of others, regarding the Law of the Excluded Middle. His own statements sometimes lack complete clarity, and even the expositions of Dresden mentioned above may not be wholly obvious. Certainly the attitude of Brouwer was not clear to many of his opponents. Thus Barzin and Errera obviously misunderstood him, as they themselves state in a later article.[5]

That Brouwer rejects the law is quite obvious. His reasons also seem not hard to find for one who wills, and they certainly appear in the examples which he cites.[6] One such example is to know whether there exists a number k which expresses the number of digits in the number π at which, for the first time, the sequence 0123456789 begins in the decimal representation of π. It is not provable at present that k exists, for such a sequence does not occur in the decimal representation of π as far as its expression has been calculated.

It is then obvious that Brouwer is thinking, as others are thinking, that it may not be possible to arrive at a demonstration of a proposition p, or of its denial p′, by a finite sequence of syllogisms, without the use of the law mentioned. If not, there may be a proposition not provable, whose denial (p′) is also not provable. This may even be the case if argu-

[3] *Rend. Circ. Mat. de Palermo*, 11: 154–164. 1897.

[4] See Brouwer (trans. by Dresden), *Bull. Amer. Math. Soc.*, 20: 81 *ff.* (1913); Brouwer, *Jahresbericht D. M. V.*, 28: 203; 1920); Brouwer, *Zeitschr. für Math.*, 154: 1 (1925); Pierpont, *Bull. Amer. Math. Soc.* 34: 23–53 (1928); Dresden, *Bull. Amer. Math. Soc.*, 30: 31–40 (1924) and 34: 438–452 (1928).

[5] *Acad. de Belgique, Classe des Sciences*, 13: 56–71 (1927); *Arch. Soc. Belge Philos.*, pp. 3–26. 1928–29.

[6] *Zeitschr. für Math.*, 154: 1. 1925.

ments that are equivalent to the use of an infinite number of steps are allowed. Such propositions need not be *absurd,* as some have claimed: the preceding example does not introduce any new concepts, and is within what is sometimes called the "universe of discourse"; the question involved can not be called absurd, or rejected; indeed, it is entirely thinkable that the question can be answered.

The immediate reply of many persons is that the proposition that k exists must either be true or else false, on the basis of "ultimate" truth. This is, indeed, precisely the Law of the Excluded Middle. If the topics discussed form a part of the concrete universe, thought of as externally real, such a position would be justified. In how far such a position is tenable will be discussed in the next section. The belief in a real external existence of the objects and relationships under discussion is undoubtedly the basis for the law, as it is, no doubt, the basis for all logic. Again, as Dresden has remarked[7] in his discussion of Brouwer's position, the law would be granted if the proposition dealt with a finite system. That Brouwer's own statements are not particularly clear on these matters will appear from the quotations which Dresden makes in the article just cited, and from Dresden's own comments.

4. Reality. The External World

As I have just stated, a crucial element in any such discussion is the meaning of the words "true" and "false," and the associated question of the conceived real existence of the concepts and relationships under discussion in the real external world. It may be as well to grant at once that the law hold in an *a priori* sense in such an *a priori* system.

On the other hand, so far as I know, no modern school of mathematical thought, least of all the postulational or formalistic school, would accept such a tenet. Beginning with the discussions of the parallel axiom of Euclid, the whole course of mathematical logic has tended strongly toward the abandonment of the claim that an axiom or a postulate necessarily represents the realities of the external universe. Thus every present school of mathematical thought, in so far as I know, would accept either Euclidean geometry or any one of the non-Euclidean geometries, with no claim that one of them more than any other represents the realities. Thus, while I have not yet discussed the different schools nor the differences that exist in their fundamental thinking, I may assume that no one of these schools would attempt to base its systems on a claim of reality. The nearest approach to such a claim would be the school associated with Brouwer himself, but since he freely accepts the different geometries mentioned above as equally valid, for example, it is clear that any claim of the intuitionists for the reality of their systems would be quite limited; to this I shall return.

If the claim for reality, the claim that the objects and the relationships under discussion form a part of an existent external world, disappears, then the *a priori* nature of "truth" and "falsity" of propositions also disappears, and with it disappears also the *a priori* assumption that any proposition is either true or else false, that is, the Law of the Excluded Middle. To reestablish it, we need at the least a clear agreement on the meaning of the words "true" and "false"; and we need new assurance of the validity of the law.

That a confusion of thought arises continually from the tendency of the human mind to revert to the primeval definitions or *a priori* conceptions of "true" and "false" appears very frequently in the literature. Thus, such a careful thinker as Church,[8] in his keen criticism of Barzin and Errera, says of the Brouwer position: "The latter point" (a statement essentially quoted from Brouwer) "depends, of course, on identifying the truth of a proposition with the possibility of proving the proposition. But it seems more in accord with our usual ideas to think of truth as a property independent of our ability to prove it. Consequently we prefer to take the truth of a proposition merely as an undefined term subject to certain postulates, among them, if we choose to include it, the law of the excluded middle." While I must admit at once that Church's position is entirely tenable and logically sound, and while it is only just to Church to admit that he says elsewhere that one may also choose to deny the law, his position seems to be to avoid trying to define "true" and "false" in order to save the Law of the Excluded Middle. One must admit the correctness (logically) of his statement, since what he proposes is precisely what is done in the system of Principia, as we shall see. To admit that such a system is a possible system of logic, however, is far from admitting that it is the only system of logic; and to assert that such a system (*e.g.,* Principia) is "more in accord with our usual ideas," that is, with the *a priori* conception of "true" and "false" in an external world of realities, seems strange to one to whom the rudiments of the Principia are known.

Still closer to the *a priori* conception of "truth" is the position taken by Barzin and Errera. In the second article just cited, they criticize the definition given by Levy, which we shall mention in a moment. They say (page 5): "M. Levy . . . defines a truth or falsity which he calls *brouwerian,*" and they speak of the "usual" sense of truth and falsity, without further comment.

[7] Dresden, *Bull. Amer. Math. Soc.,* 30: 39. 1924.

[8] Church, *Bull. Amer. Math. Soc.,* 34: 75–78. 1928.

The definition given by Levy[9] deserves attention. Although he calls it "brouwerian," he has stated also that it does not agree precisely with Brouwer's own view. Levy says that a proposition is "true" if it is possible to demonstrate it (obviously without the use of L. E. M.), and "false" if it is possible to prove its denial p′. I myself had used essentially these same definitions[10] in a paper presented to this society, and I have been told by Weyl, in conversation, that this is probably Brouwer's point of view, though I am not aware that he has stated it distinctly and clearly.

The effect of such a definition upon the law (L. E. M.) is clear enough. If a proposition p is not "true," does it follow that it is "false"? This now has a distinct meaning, at least. It means: if p can not be proved (in a finite number of steps and without L. E. M.) then p′ can be proved. Of this there is no present guarantee. Hence the law (L. E. M.) has no present validity. What further effects such a position may have upon (say) the mathematics of a postulational system, and in how far the validity of the law (L. E. M.) may be secured in such a system, will be discussed later.

As a result of the present discussion, it is sufficient to point out that a crucial element in any treatment of these matters is to determine what the author means by "truth," that is, whether he reverts to the *a priori* concept based on a claim of reality; or assumes, with Church, what is essentially the Principia position, that "truth" is an undefined property; or uses what I shall call, with Levy, the "brouwerian" definition of "truth"; or gives some other definition of the term. Clearly, the law (L. E. M.) has totally different significances in these various cases.

5. The Formalistic or Postulational Method

It is probable that the postulational or formalistic view-point is the one most familiar to my audience, but a precise statement of it is not easy to give. A clear statement is given by Bernstein,[11] and I will not repeat it. Many other authors have given clear descriptions. One given with a clear consciousness of the existence of Brouwer's ideas is that of Dresden,[12] which is nevertheless, in its essence, a description of the postulational view-point.

I wish to refer hurriedly to some of the fundamental ideas present in modern discussions of any postulational system, but I shall assume that you are familiar with them. It is usual to prove that the postulates stated are *independent,* that is, that no one can be deduced from the others; that they are *consistent,* that is, that they contain no hidden contradiction, which is done (ordinarily) by exhibiting a set of objects and operations or relations whose existence is supposed known and which satisfy all the postulates. It is desired also to show that the system of postulates is *complete,* that is, that no new postulate may be added to the set without the introduction of a new undefined concept; and that they are *categorical,* that is, that two systems that satisfy the postulates can be made to correspond isomorphically.

The present status of this school of thought is also known to you perhaps better than is that of any other school. Its results have been quite successful in many fields of mathematical thought, and the extent to which such examination of systems of postulates has been carried surely exceeds any estimate which might have been made, say, in 1900. One serious bar remains. In general, the proofs of consistency have been made on the assumption of the soundness of the postulates of arithmetic. These postulates themselves have been discussed extensively, and most strenuous efforts have been made to prove that they themselves are consistent. Hilbert and his followers have been most hopeful that final success would be achieved, and a partial success has been attained by Hilbert himself,[13] von Neumann[14] and others. The means employed by Hilbert and others, however, particularly the employment of arbitrary marks, seem of doubtful validity to many, and one must hesitate to announce that a final success has been attained.

The present status, then, remains that we must assume the validity of the postulates of arithmetic, in some form. We need, indeed, thus much of assumed *reality,* or at least assumed *consistency.* If it be an assumption of *reality,* then the L. E. M. would have a certain *a priori* validity based on that assumption; but even that would not of itself carry through to other sets of postulates based upon this one, since a proof of consistency by no means demonstrates reality.

As for "truth," we may assume, with Church, that it is an undefined property, and we may adjoin (see Bernstein, *loc. cit.*) some such set of postulates for logic as that of the Principia. To do so, however, is clearly an act of *choice,* not of necessity; and this Church clearly indicates.

On the other hand, it would appear more in keeping to call these things "true," just as we call the postulates themselves "true," not because of an instinctive belief in their ultimate reality in an external world, but simply because they follow from the postulates.

[9] *Acad. de Belgique Classe des Sciences,* 13: 256–266. 1927.

[10] *Bull. Amer. Math. Soc.,* 34: 436. 1928.

[11] *Bull. Amer. Math. Soc.,* 37: 484. 1931.

[12] *Bull. Amer. Math. Soc.,* 34: 442. 1928.

[13] Hilbert, *Jahresbericht D. M. V.,* 8: 1900; *Math. Annalen,* 78: 405. 1918; *Abh. Hamburg Univ.,* 1: 157. 1922; *Math. Annalen,* 88: 151. 1922–23, and 95: 161. 1926. Also references given by Dresden, *Bull. Amer. Math. Soc.,* 34: 440. 1928.

[14] v. Neumann, *Math. Zeitschrift,* 26: 1 (1927), and *Math. Annalen,* 154: 219. 1925.

This is indeed no other than the position that I myself have taken, that Levy took, and that I have called, with Levy, "brouwerian." It seems to me, however, not primarily brouwerian; rather, it seems to me very decidedly in accord with the very spirit of the postulational school. In that case, however, some support would have to be given to establish the validity of the L. E. M., since it ceases to be obviously true.

If we could prove the set of postulates have the property that any proposition p can be proved, or else its denial p′ can be proved, from the set of postulates, we should have turned the trick. If we can not do this, then of what avail is it to have adopted rules of logic which essentially beg the question? Such rules of logic *do* essentially beg the question, for, after all, what we want to know is precisely whether or not we can prove p or else p′. To assume that we can always do one or the other of these things, or, what amounts to the same thing, to say (in no matter how disguised a form) that p is to be called "true" not only when we can give a direct proof of it, but also whenever we can not prove p′, seems strangely out of accord with the spirit of the postulational method. It is, however, logically possible to do this.

A dilemma to which we are led if we take this position has been stated by Church in his discussion of the Zermelo postulate.[15] Here he himself is dealing with a possible *addition* to the laws of logic, and, since alternatives present themselves, it is obvious that we can not decide all questions until some such additional law has been assumed. Realizing the relation of this question to the L. E. M., Church states (p. 186) that the fact that we might be led, after assuming one or the other of two additional postulates, to different conclusions regarding the same proposition, he concludes that we should then have not a violation of the L. E. M. but rather two universes of thought, each self-consistent, proceeding forever side-by-side, perhaps after the manner of the two universes of thought contained in Euclidean geometry and one non-Euclidean geometry. It seems obvious, however, that *just before* the additional postulate is chosen, we should certainly be in possession of a consistent set of postulates for which the L. E. M. does not hold. Nor is it clear how one may know when this situation exists. For example, before the explicit statement of the Zermelo postulate, would it have been clear that the L. E. M. did *not* then apply to the system as it was? Let me say distinctly, however, that the remark of Church seems to me to be sound in so far as it goes.

6. The Intuitional School

I have discussed already two phases of the work of Brouwer, namely, his attitude toward the L. E. M. and his attitude toward proofs or definitions that are based effectively upon the use of an infinite number of steps. Neither of these has a specific relation, however, to the intuitional method, in the sense that one may adhere to either or both of these other principles without attaching himself thereby to the intuitional school; indeed, as I have pointed out, many mathematicians of note have done so, without participating thereby in the intuitional movement.

Brouwer has stated his position on intuitionalism as the proper basis for mathematical work in numerous articles cited above. Two quotations from him that are given by Dresden[16] seem to give in brief space something of his view. "This intiution," says Dresden, "upon which not only mathematical thinking, but all intellectual activity is held to be based, is found in the abstract substratum of all observation of change, 'a fusion of continuous and discrete, a possibility of conceiving simultaneously several units, connected by a *between* that can not be exhausted by the interpolation of new units.'" And again: "It [the fundamental intuitive concept of mathematics] manifests itself in the intuition of time, which makes possible 'repetition, as being object in time, and again object.'"

Without pretending that this represents the whole of the point of view of the intuitionists, I may say that one concept which seems to stand out clearly is the acceptance on the basis of intuition, of some essential properties of *time*. In particular, the indefinite divisibility of it, the occurrence of a time between any two other times, appears; and the idea of *unit* is emphasized. These are indeed fundamental properties of pure number, as it is conceived in arithmetic. The recognition of *time* as a primordial prototype is not at all novel: Hamilton defined *mathematics* as "the science of pure time," and many another has done his fundamental *intuitional* thinking (which we may grant all mathematicians *must* do in some way) in terms of *time*.

While I reiterate that this does not exhaust the views of Brouwer on this field of thought, it does appear to me that there is here a common ground which I have not seen emphasized, as between intuitional and postulational thinking: it is that in both, if I have not too much distorted the facts, the essential properties of *time* (that is, of ***real number***), are at present assumed to be valid upon the basis of our intuition, for I have pointed out that the present state of the postulational school is essentially just this.

At at least one other point, however, the two schools of thought part company in a more decisive manner. Brouwer contends that logic is not a sure guide toward the building-up of mathematical thought, and that the existence of a variety of possible logical sys-

[15] *Trans. Amer. Math. Soc.*, 29: 178–208. 1927.

[16] *Bull. Amer. Math. Soc.*, 30: 32. 1924.

tems leads one to feel that results obtained by one such system may not be wholly convincing. This position has been clearly stated by Boutroux.[17] It is admirably demonstrated, in fact, by the results which have been obtained by the use of the Zermelo postulate, as contrasted with results obtainable with no such postulate, or with a different one. I think that Brouwer would hold that the intuition must be called upon to decide when such a situation arises. That few of us would be able to do this with any degree of confidence, is, perhaps, not a valid objection. Certainly the formalistic or postulational school would not agree to such a procedure, except, perhaps, in an extremely restricted and narrow form. To what extent the modern developments of logic tend to confirm Brouwer's views, we shall see presently.

I have referred in what precedes chiefly to the Zermelo postulate because it is in some senses the best known case of an addition to the preceding logical system. There are others, however, which are nearly if not quite as prominent in the minds of mathematicians. Among these, I may mention the proposed postulate of Russell[18] that "whatever involves all of a collection must not be one of the collection," and the proposed postulate of Hilbert[19] to sanction the use of complete induction in the transfinite case. These notable instances of additions to the logical system, and their possible alternatives illustrate in themselves the possible variety of logical systems of which Boutroux speaks.

While I make no claim to have exhausted the views of intuitionists, I have presented some which seem to have their support. It is to be noted that the advances in logical theory tend rather to support them than otherwise, but perhaps not in precisely the manner that was intended by their originators.

7. Logistics. The Principia

The logistic school has attempted to introduce a symbolism to replace ordinary language, in order to avoid the errors traceable to the ambiguities of ordinary language, and to express mathematics essentially as a part of logic by means of this symbolism. A first comprehensive attempt was begun by Peano in the "Formulaire de Mathématique" (Vol. 1, 1895), which was extended and completed in four volumes during the years 1895–1903 by Peano and several other Italian mathematicians. A still more comprehensive and exhaustive treatment began with a paper by Russell in the *American Journal* in 1908, and led to the monumental work "Principia Mathematica," by Whitehead and Russell (2d ed., 1925–27), to which reference has been made above. It was thought by the authors and by many followers that the procedure followed was subject to no errors, and that the resulting system was indeed final. Many mathematicians, however, including most German mathematicians, remained skeptical, and it is true that the authors felt obliged to introduce fundamental changes in the second edition, largely due to the work of H. M. Sheffer. A brief statement of the essential purposes and methods of the "Principia" is given by Bernstein,[20] and I may refer any who are not familiar with its nature to that article In his review of "Principia,." Bernstein pointed out certain difficulties of interpretation, and he has insisted upon these difficulties in his later articles[20] A simple instance is that the symbol "p" is often read "p is true," in spite of a warning by the authors of "Principia" against doing so. There are also certain "informal" statements in the "Principia" which were intended to clarify the more formal ones; but it appears that there are real differences between the "formal" and the "informal" systems, so that it may be said that the "Principia" consists of two different systems conducted simultaneously.

The definition of *implication* is stated essentially in the form: "p implies q if it is true that either not-p is true or else q is true." This definition is entirely sound, but it departs fundamentally from the Aristotelian system. Thus a false proposition implies any proposition whatever; and a true proposition implies any other true proposition. For this reason, the authors of the "Principia" took the position that the usual concepts of *independence* and of *consistency* do not apply to their own system, since any (true) postulate must imply any other (true) postulate. Bernstein has shown that this is not the case, however, by reducing the "Principia" assumptions to the form of postulates for a Boolean Algebra, since the definition of implication stated above essentially divides all propositions into two classes, which may be identified with the symbols 1 and 0 of a Boolean Algebra, for which postulates paralleling those of the "Principia" are then stated. These postulates are subject to examination for independence and for consistency, so that the original "Principia" postulates are also. It also proves possible to state the entire system in terms that make it a strict mathematical science in the sense of the postulational method. Another comprehensive survey of the situation is in type and will be published in the January, 1933, issue of the Transactions of the American Mathematical Society by E. V. Huntington. His system differs from that of Bernstein chiefly in the introduction, not only of the class K of all propo-

[17] Boutroux, "L'Idéal des Mathématiciens." 1908.

[18] *Amer. Jour. Math.* 30: 225 (1908); Whitehead and Russell, "Principia Mathematica" (2nd ed., 1925), p. 58.

[19] Hilbert, *Math. Annalen*, 88: 151. 1922–23.

[20] Bernstein, *Bull. Amer. Math. Soc.*, 32: 711–713 (1926); 37: 480–488 (1931); 38: 388–390 (1932); 38: 589–593 (1932).

sitions, but also of another class T of propositions, a subclass of K, in terms of which the postulates are stated. The independence and consistency of these postulates are examined with great care. It is shown that the resulting system is equivalent to the "informal" "Principia" system, but the inherent difficulty of deriving it from the "formal" "Principia" system again manifests itself. Quite informally, one may think of T as the subclass of *true* propositions.

In either Bernstein's or Huntington's systems, and therefore in the "informal" "Principia" system, the L. E. M. follows from the postulates, though this is not the case for the "formal" "Principia" system. However, no immediate conclusion as to the universal validity of that law can be said to follow, since it is here simply postulated. The only conclusion is that the "informal" "Principia" is a dichotomy; that is, it is a two-valued system. This does not show that every mathematical system must also be dichotomous; it shows only that the "informal" "Principia" system is not applicable to a mathematical system unless that system is shown to be a dichotomy. This is quite a different statement.

8. Other Logical Systems. Abandonment of L. E. M.

Despite the monumental nature of the "Principia," it follows from what precedes that we must again face the question as to whether or not the L. E. M. is of universal application. The recent work of Bernstein, Huntington and others have not altered the inherent nature of the "Principia"; they have simply refined it, and have transformed it into a mathematical system, freed from essential disagreements between "informal" and "formal" system.

Attempts have been made to demonstrate the law. I have referred to the attempt of Barzin and Errera. Many replies were made to this attempt, of which two that are noteworthy are the papers of Church and Levy, in which the difficulties of the attempted argument are clearly established. Indeed, if the L. E. M. could be proved from the remaining standard laws of classical logic, it would follow, of course, that there would be no necessity of stating it. In a sense, therefore, any such attempt was predestined to fail, if any statement of the law is needed.

In 1930, however, a very definite settlement of all such controversy was given by Lukasiewicz and Tarski,[21] who actually set up a trichotomy which satisfies every requirement of implication, and which possesses a strict table of implications, including a *middle,* which I may call "uncertainty," as well as the customary "true" and "false." An excellent account has been given by C. I. Lewis in *The Monist* for October, 1932. It appears from their work, and from some later papers, that it is perfectly possible to set up and to operate perfectly sound systems which have just as definite "implication" systems as that of the "Principia," which have any desired number of intermediate "truth-values." One such which has been worked out with care is a quadruple system whose "truth-values" I may call "true," "probable," "improbable" and "false." Indeed the resulting system bears at least as close a relation to the popular interpretation of these four concepts as does the "Principia" to the popular interpretations of "true" and "false."

We are then obliged to conclude, with Lewis, that the L. E. M. is not "writ in the heavens," that it is not a law of universal application in logic. Rather it "reflects our stubborn adherence to the simplest of all possible modes of division, and our predominant interest in concrete objects as opposed to abstract concepts." These words of Lewis should have strong import to mathematicians, for in mathematics we deal habitually with just such abstract concepts.

9. Examples. Theory of Probability

Lewis, in the paper cited above, and Lewis and Langford, in their "Symbolic Logic" use the phrases "certainly true," "certainly false," "doubtful" as names for the "truth-values" of a trichotomy. I have insisted above upon such situations as actually existent, useful and commonly employed for practical ends, with at least as close adherence to the theoretical systems as obtains in the dichotomy "true" and "false" in the logic of ordinary reasoning.

It would seem that any desired scheme of "truth-values," however numerous, would find again a parallel in human thinking. The quadruple system "true," "probable," "improbable," "false," mentioned above, is such a case; though again I must insist that the precise rules for implication would be followed no more than in the case of ordinary reasoning. To obtain a similar parallel to any number of "truth-values," it is only necessary to appeal more openly to the theory of probabilities, and to assign to definite degrees of probability from 0 to 1 various "truth-values," that is, various gradations of certainty. I am attempting only to point out that such systems are by no means bizarre systems (or at least no more bizarre than is the "Principia" system), for something much akin to them is actually present in our minds, and forms habitually a basis for a great part of our actual thinking.

Nor is the limiting case of all this at all bizarre. It is quite legitimate to set up a scale of probabilities, and we do commonly do so, in which each rational

[21] *Comptes Rendus Soc. de Varsovie,* 23, III: 51–77. 1930.

proper fraction expresses a degree of probability, with the extreme limits 0 and 1, which express impossibility and certainty. The parallel logical system with an infinite number of graduations in "truth-values" from "false" to "true" appears to be just as feasible. Finally, a continuous scale might be established in either the popular sense of the theory of probability, or in the exact "implication" sense of a strict logical system. That the theory of probabilities may be thought of, at least informally, as constituting in itself a scheme of logical thinking in which there are many gradations of "truth-values" may form a clue toward rational appreciation of logical systems in which the L. E. M. is violated. It is unnecessary, however, to pretend to precise correspondence in this case, any more than there is precise correspondence between the "Principia" system and ordinary reasoning, for in each case independent and satisfactory proof exists that the logical system in question is legitimate.

10. Conclusions

I have attempted to outline in its major details the development of the logic of mathematics in recent years. If it appear to any that the acceptance of such ideas will destroy mathematics or parts of it, I would point out that a certain amount of discarding of what has been is necessary in order to assure progress. If we are to deny new developments whenever they require relinquishment of the ideas of the past, we shall be serving not truth but only our vanity.

It is indeed true that many famous mathematicians have given up in despair. Cantor, Dedekind, Frege, all consciously accepted defeat, and bitterly. I have mentioned the protest in a former generation of Weierstrass; in our generation, of Hilbert; these great men cry out that great portions of mathematics will fall if such ideas prevail. True, but that is no reason why they must not prevail if they be correct.

Nor is despair the note of all scholars. Brouwer, Weyl, Poincaré, show no sign of pessimism. They, and many lesser men, feel only that we may go on to new conquests of truth. New minds will arise who will carry mathematical truth in the next generation not to destruction, but to yet greater heights, to new and higher ideals of rigor.

For young men, there is a challenge in all this. Not that we can presume that Rome can be rebuilt after the conflagration in a single day. For a long time we must proceed as we now seem to be: asserting that our results are valid if we postulate the L. E. M. along with our other postulates. While this is a fundamentally weak situation, in view of our inability to show that our postulates are categorical, it is at worst defensible in the present state of our knowledge. We must proceed slowly. A vast amount of work that should and must be done lies in exploring the whole of mathematical literature to determine which theorems in a given field require the use of the L. E. M. for their demonstration, in the form in which we now do demonstrate them. Next, we may find out how to give better proofs, in which that law is not assumed, at least in some cases. On the basis of any philosophy, such discoveries would be valuable. For a reconstruction of mathematics they are quite necessary.

Can definitions and existence theorems be revised in every field, at least in some cases, so that the concepts discussed can be actually arrived at in a finite number of steps?

Such reconstructions are in fact already in progress. Not only Brouwer, but many others who have seen the need for such investigation, irrespective of an ultimate acceptance of all that now appears to be true, have been working toward these very goals. One need by no means accept the intuitionist point of view to see the value of such reconstruction. If it be only partial in our time, we should not fear the ultimate future. Generations yet unborn will doubtless face still some of our doubts, some of our problems.

There is, no doubt, some sense of tragedy in this, as in every previous instance of advancing ideals of logical exactness. We too lay down our task, unfinished. I shall only paraphrase the sentiments of great mathematicians of the past who have seen their labors end incompleted, if I quote to you some of the lines of a poem of Kipling which I treasure deeply, "The Palace"; in it, the master builder, after years of proud effort, lays down his task at last, saying:

When I was a King and a Mason,
In the open noon of my pride,
They sent me word from the Darkness,
They whispered and called me aside;
They said: "The *end* is forbidden."
They said: "Thy use is fulfilled;
"Thy Palace shall stand as that other's,
"The Spoil of a King who shall build."

I called my men from my trenches,
My quarries, my wharves, and my shears,
All I had wrought I abandoned
To the Faith of the faithless years.
Only I cut on each timber,—
Only I carved on each stone:
"After me cometh a builder;
"Tell him I too have known."

OBITUARY

EDWARD BRUCE WILLIAMSON

EDWARD BRUCE WILLIAMSON, entomologist and naturalist, died at Ann Arbor, Michigan, on February 28, 1933, at the age of fifty-five. His boyhood days were spent on a farm near Bluffton, Indiana. After graduating from Ohio State University in 1898, he was assistant curator at the Carnegie Museum, 1898–99, and a fellow at Vanderbilt University, 1900–01. From 1905 to 1930 he served as president of the Wells County Bank at Bluffton, and since 1930, as research associate at the Museum of Zoology, University of Michigan.

His first paper on the *Odonata* appeared in 1897 and in the interval between then and his death he published more than 65 papers on this group of insects. Much of the work was done before and after banking hours. While the papers were mainly of a systematic nature, they also contain much data of a general biological nature observed during his extensive collecting trips in the United States, Central and South America. The value of these contributions is attested by the high esteem in which he was held by entomologists the world over. In addition to his work as an entomologist he was one of the pioneer breeders of iris. He developed and placed on the market many of the best varieties and his interest in this field was maintained until the end. His interests, however, were not limited to these two fields. In his earlier years he had collected birds' eggs and during his boyhood days he was much interested in pets. In fact, he was interested in all living things, a naturalist in the true sense of the word.

The things which have been said, however, give to the stranger no real concept of Bruce Williamson. One had to know him personally in order to appreciate him. He was a rare personality; an amateur in spirit and enthusiasm, energetic, stimulating alike to the non-scientist as well as the scientist. So contagious was his enthusiasm for natural phenomena that he was able to interest either children or adults in the things he was doing. It was literally impossible to be around him without getting an insect net to chase dragon-flies or without planning an iris garden. One could not remain static in his presence, because of his seemingly inexhaustible supply of energy. Perhaps, because of the use of so much energy, his death came somewhat prematurely, but who can say that he did not live more richly and abundantly than those of us who creep at a slower pace. We who had the privilege of knowing him intimately prized his friendship in a way difficult to express in words, and hence his loss to us is the greater. The world would be richer if we had more naturalists like him.

FERNANDUS PAYNE

RECENT DEATHS

THE death is announced of Dr. Frank Mann Life, physicist, dean of the College of Letters, Arts and Sciences at the University of Arizona. He was in his sixty-third year.

JOHN LEMUEL STONE, emeritus professor of farm practise at the New York State College of Agriculture at Cornell University, died on March 8. He was in his eightieth year.

CHARLES E. EVELETH, a vice-president of the General Electric Company and formerly manager of the Schenectady plant, died on March 25, at the age of fifty-seven years.

DR. ROYAL EDGAR JEFFS, associate professor of botany in the University of Oklahoma, died on February 17, at the age of fifty-three years.

ARTHUR BLISS SEYMOUR, since 1886 assistant in the cryptogamic herbarium at Harvard University, died on March 29, at the age of seventy-four years.

DR. FERNANDO WOOD MARTIN, formerly vice-president of Randolph-Macon Woman's College and for thirty-six years head of its department of chemistry, died on March 22, at the age of sixty-nine years. He retired in 1929 as emeritus professor.

THE REV. FREDERICK L. ODENBACH, S.J., for the past forty years meteorologist and seismologist at John Carroll University, Cleveland, Ohio, died on March 15, at the age of seventy-five years. It was through Father Odenbach's influence and initiative that seismographic equipment was installed and stations were organized at fifteen Jesuit colleges and universities in the United States and Canada during the years 1909 to 1912.

DR. WILLIAM HENRY PORTER, professor emeritus of pathology and internal medicine at the New York Post-Graduate Medical School, died on March 27. He was in his eightieth year.

DR. JOHN MILLAR THOMSON, emeritus professor of chemistry at King's College, London, died on March 22, at the age of eighty years.

SCIENTIFIC EVENTS

INTERNATIONAL STANDARD FOR THE OVARIAN FOLLICULAR HORMONE

DURING the past summer the Health Organization of the League of Nations held a meeting on the standardization of the estrus-producing hormone. Sir Henry Dale presided over the conference, which was composed of A. Butenandt, E. A. Doisy, E. Laqueur, G. F. Marrian and A. S. Parkes. In an abbreviated

form the most important points of the agreement were:

(1) That a material standard preparation should be preserved as a central standard of reference.

(2) That this material standard should consist of the hydroxy-ketonic form of the hormone.

(3) That the various countries will be invited to contribute samples which will be combined to form the standard preparation.

(4) That the unit will be the specific estrus-producing activity contained in 0.1 gamma of the standard preparation.

(5) That by the term "estrus-producing activity" is to be understood the power of producing in an adult female animal completely deprived of its ovaries an accurately recognizable degree of the changes characteristic of normal estrus, *i.e.*, the changes in cellular contents of the vaginal secretion of the rat or mouse.

(6) That the comparison of an unknown preparation with the standard can only be accurately made if the conditions of administration and interpretation of results are identical.

(7) That a bio-assay procedure, which has been proved to be capable of determining the equality of activity with an error not greater than plus or minus 20 per cent., should be used in comparing a preparation with the international standard.

(8) That proper precautions will be observed in the preparation and storage of the international standard. The international standard will be kept at the National Institute for Medical Research in London, but a portion will be available for distribution in each country.

The adoption of an international standard should solve many of the vexing problems of bio-assay of the estrus-producing hormone. In the past, many modifications of the original Allen-Doisy bio-assay procedure have been introduced. This has led to a great many different units, since so many different factors alter the absolute weight of the hormone required to produce estrus. The result was that no investigator could compare his results with those of another investigator with any feeling of assurance of accuracy. With the establishment of an international standard it is hoped that investigators will make use of this standard preparation and that pharmaceutical houses will also label their products in terms of the international unit.

E. A. D.

THE ELEVENTH INTERNATIONAL SYMPOSIUM ON PHYSICS AT THE UNIVERSITY OF MICHIGAN

THE eleventh International Symposium on Physics at the summer session of the University of Michigan will be held as usual. Owing to reduced university appropriations its support has been provided for from private sources.

The foreign lecturers this year will be Professor Niels Bohr, of the University of Copenhagen, Nobel laureate, the founder of modern atomic physics, and Professor Enrico Fermi, of the Royal University at Rome, authority on quantum theory. Professor John H. Van Vleck, of the University of Wisconsin, specialist in the theory of magnetism, will also lecture, as will Professors Ernest F. Barker, David M. Dennison and George E. Uhlenbeck, of the university.

Dr. Edward H. Kraus, dean of the summer session, states that the symposium brings together annually leading native and foreign authorities who conduct classes and meet together with their students in give and take discussions of both the theoretical and experimental aspects of the laws of the atom, infra-red rays, the revelations of the spectroscope and like problems.

In recent years physicists from outside the United States who have attended the symposium include Professors Erhenfest, of Leiden; Heisenberg, of Leipzig; Sommerfeld, of Munich; Pauli, of Zurich, and Kramers, of Utrecht, as well as Brillouin, of Paris; Dirac, of Cambridge, and Milne, of Oxford.

GEOLOGY AND GEOGRAPHY AT THE CHICAGO MEETING OF THE AMERICAN ASSOCIATION

SECTION E of the American Association for the Advancement of Science will meet in Chicago on Tuesday, Wednesday and Thursday, June 20, 21 and 22 in conjunction with members of the Geological Society of America, the Geographical Society of Chicago and other affiliated organizations. The Hotel Southmoor at Sixty-seventh Street and Stony Island, will serve as headquarters. An informal dinner will be held on Tuesday evening at the University of Chicago and will be followed by a lecture on "Variation and Evolution in Earth History" by Professor Albrecht Penck, Berlin, who with Professor J. J. Sederholm, Helsingfors, Finland, is among the foreign guests who will address the members of this section and their friends. The meetings will be held at the University of Chicago and at the Field Museum of Natural History.

The Tuesday morning session will be devoted to papers on general geology and, if necessary, the reading of papers will be continued on Tuesday afternoon. On Wednesday afternoon there will be a joint session with the physicists, chemists and astronomers, devoted to a symposium on "The Measurement of Geologic Time." Among those who will participate in this symposium are: A. F. Kovarik, A. C. Lane, H. Schlundt, A. V. Grosse and W. D. Urry. For the Thursday session, which will be held jointly with the Geographical Society of Chicago, a program of papers on geography is being arranged by Professor Charles C. Colby, of the University of Chicago.

Local arrangements are in charge of Professor D. Jerome Fisher, of the University of Chicago. Hotel reservations should be made directly with the Hotel Southmoor. Titles and abstracts of papers for the program on geology should be submitted to Professor Kirtley F. Mather, Harvard University, Cambridge, Massachusetts, prior to May 10.

AWARD TO PROFESSOR H. C. SHERMAN

THE medal of The American Institute of Chemists, presented annually for outstanding service to chemistry in America, has been awarded this year to Dr. H. C. Sherman, head of the department of chemistry at Columbia University, according to an announcement made by President Henry G. Knight from the office of the institute. The award is made in recognition of Dr. Sherman's food researches, and his services to the profession through the training of chemists.

One of the pioneers in food chemistry from the time he first began to teach at Columbia in 1898, Dr. Sherman has been prominent most recently for his studies of the effect of diet on longevity, carried out in collaboration with the Carnegie Institution of Washington. He was the first to insist on quantitative analysis of iron and other minerals in foods; and he has also carried out research on enzymes and on the vitamin values of foods and the significance of vitamins in nutrition.

Dr. Sherman's work for fifteen years with the food supply committee of the New York Association for Improving the Condition of the Poor has resulted in a marked improvement in the food and nutrition conditions of the average New York family.

Describing Dr. Sherman's work as "use-research," Dr. Knight called for increased scientific efforts to teach people how to live intelligently. "Industry can always afford to pay for the discovery of products it can sell," Dr. Knight said, "but only educational institutions, subsidized foundations and state and national governments can improve living conditions by showing people how to use the things they already have. The individual can not pay for his own nutrition research. Instead, he pays for medicine and doctors, after he has guessed wrong."

Last year's medalist of the American Institute of Chemists was Dr. Charles H. Herty, whose newly discovered process for making paper from the Southern slash pine has just turned out its first mill run of newsprint. Other past medalists include Andrew W. Mellon and Richard B. Mellon, honored for establishing the Mellon Institute for Industrial Research; the late George Eastman, who made fine organic chemicals available to the chemists of the country, and Mr. and Mrs. Francis P. Garvan, who established the Chemical Foundation.

The medal will be presented to Dr. Sherman at the annual meeting of the American Institute of Chemists, to be held in New York in May.

PROFESSOR EINSTEIN AND THE PRUSSIAN ACADEMY

ACCORDING to a special cable to the *New York Times*, the Prussian Academy of Science accepted on April 1 the resignation of Professor Einstein and announced that he was renouncing the Prussian citizenship conferred on him in 1913 on his admission to membership. He was born in Ulm, Germany, but subsequently his family moved to Switzerland and he became a Swiss citizen.

Professor Einstein has also withdrawn from the management of the Kaiser Wilhelm Institute, which recently appealed in the United States for funds for the construction of a new physical laboratory.

The Prussian Academy, its statement declares, "learned with indignation of Professor Einstein's participation in atrocity propaganda in America and France" and immediately demanded an explanation. His reply, it is stated, was that he "could no longer serve the state under the present government."

"The Prussian Academy," the announcement continued, "deplores Professor Einstein's propagandist activity abroad the more since, of old, the academy and its members have felt themselves intimately conjoined with the Prussian State and for all the strict reserve they have maintained respecting political questions, have always emphasized and preserved the idea of nationality. The academy, therefore, has no reason to regret Professor Einstein's retirement."

It is further reported that funds and securities in a Berlin bank have been seized by the Nazi Government "to prevent their use for treasonable purposes."

Professor and Mrs. Albert Einstein arrived at Antwerp aboard the liner *Belgenland* on March 28. Mayor Camille Huysmans went aboard to welcome them. Professor de Groodt, who lives near Antwerp, will be their host for several days. They plan to remain in Belgium four or five months, taking a cottage at Coq-sur-Mer near Ostend. Professor Meyer, his assistant, will join him.

SCIENTIFIC NOTES AND NEWS

ANNOUNCEMENT of the establishment of the Elihu Thomson Professorship of Electrical Engineering at the Massachusetts Institute of Technology was made on March 29 by President Karl T. Compton, at the dinner given at the institute to Professor Thomson in honor of the eightieth anniversary of his birthday.

In announcing the plan, Dr. Compton said that the institute's executive committee had voted to establish the professorship "under terms which may be expected continually to draw as the incumbent a man of such qualifications for teaching and research as shall fittingly and permanently carry forward those high ideals of creative scholarship and productive service as have so distinguished the man whose name this professorship will bear."

DR. LYMAN J. BRIGGS has been nominated by President Roosevelt to be director of the National Bureau of Standards. Dr. Briggs, who had been acting director since the death of Dr. C. K. Burgess, had been previously nominated for the position by President Hoover, but the appointment had not been confirmed by the Senate.

DR. WILLIAM I. MYERS, professor of farm management at the New York State College of Agriculture at Cornell University, has been appointed assistant secretary to Henry Morgenthau, Jr., chairman of the Federal Farm Board. Dr. Myers has been given leave of absence from the university until October.

DR. S. H. KNIGHT, professor of geology at the University of Wyoming, has been appointed state geologist, as the office of state geologist at the State Capitol, Cheyenne, has been abolished. The Legislature has provided for the creation of the Geological Survey of Wyoming, to function under the department of geology of the University of Wyoming.

DR. LYMAN J. BRIGGS, Walter S. Gifford, Admiral William V. Pratt and Dr. Alexander Wetmore have been elected trustees of the National Geographic Society. They will fill vacancies caused by the deaths of former President Coolidge, Dr. George K. Burgess, Rear Admiral Colby K. Chester and O. P. Austin. George W. Hutchinson has been elected secretary of the society.

DR. J. T. DUNN has been elected president of the British Society of Chemical Industry for the year 1933–34 in succession to Dr. R. H. Pickard. Dr. Dunn is the public analyst of Northumberland and represents the Fuel Research Board on the North Eastern Coalfields Survey.

DR. HARVEY CUSHING, Moseley professor of surgery, emeritus, at the Harvard Medical School, has been appointed an associate fellow of Trumbull College, one of the new colleges in Yale University.

THE Langmuir Award of the American Chemical Society for chemical research has been conferred this year on Dr. Frank Harold Spedding, of the University of California. The award, which carries a prize of $1,000, is for Dr. Spedding's study of the atom by spectrographic methods.

DR. CHARLES A. MOSS and Herbert D. Brown, of the General Electric Company, recently received Charles A. Coffin Foundation awards from the company. Dr. Moss was cited for his design of the centrifugal supercharger or "tamed torpedo" which reinforces airplane engines, and Mr. Brown received the award because of his work in the development of the mercury-arc rectifier. These awards, established in memory of Charles A. Coffin, first president of the General Electric Company, are granted each year for outstanding service in advancing electrical industry.

DR. DICKINSON W. RICHARDS, JR., assistant professor of medicine in the Columbia University School of Medicine, has received the Walter Belknap James Fellowship for 1933–34, in recognition of his research on "Blood Flow in Cardiac Disease."

DR. ROYD R. SAYERS, of the U. S. Public Health Service, who has been assigned to the U. S. Bureau of Mines for the past sixteen years, has been transferred back to the service to take charge of the division of industrial hygiene and sanitation. Dr. Albert Eugene Russell will succeed Dr. Sayers.

The American Physics Teacher, the initial number of which appeared in March, is a new quarterly journal sponsored by the American Association of Physics Teachers. It is an addition to the list of seven periodicals which the American Institute of Physics now publishes. Devoted to the interests of teachers of physics, the journal will feature articles on the teaching of physics and related problems, and will have departments devoted to discussion, laboratory and demonstration apparatus and abstracts. The editor is Dr. Duane Roller, associate professor of physics at the University of Oklahoma. Associate editors are: Dr. A. A. Knowlton, Reed College; Dr. C. J. Lapp, University of Iowa; Dr. Louise S. McDowell, Wellesley College; Dr. F. K. Richtmyer, Cornell University; Dr. L. W. Taylor, Oberlin College, and Dr. David L. Webster, Stanford University.

Nature writes: "With the close of 1932, Mr. T. Sheppard retired from the editorship of the *Naturalist,* having served it as editor for thirty years. As an illustrated monthly journal, designed for readers within the domain of the Yorkshire Naturalists' Union, the *Naturalist* has maintained a high standard, and in its comments and reviews it conveyed a touch of the editor's vitality and boisterousness. The occasion of his retirement was made an opportunity of presenting Mr. Sheppard with his portrait in oils by Mr. Vincent Galloway."

DR. DONALD A. LAIRD, director of the psychological laboratory of Colgate University, has been appointed to supervise the exhibit on sleep and fatigue for the

Century of Progress Exposition. The exhibit, which is being supported by the Master Bedding Makers of America, will feature the actual use of apparatus used on recent studies of sleep and fatigue at several universities, giving visitors an opportunity to take some of the tests.

THE Committee on Scientific Research of the American Medical Association has made grants to Dr. James L. O'Leary, of the department of anatomy of Washington University Medical School, St. Louis, to aid his work on the nervous mechanisms controlling blood pressure, and to Dr. Helen C. Coombs, of the department of physiology of the New York Homeopathic Medical College, for an investigation of the effects of bromides on experimentally induced convulsions.

DR. PAUL BARTSCH, Smithsonian Institution biologist, who led the Johnson-Smithsonian Deep-Sea Expedition to investigate the Puerto Rico Deep, has returned to Washington. Some of the specimens obtained by the expedition at depths extending to 3,200 feet are believed to be new. The expedition was made possible through the generosity of Mr. Eldridge R. Johnson, of Philadelphia, who placed at the disposal of the Smithsonian Institution his motor yacht *Caroline,* and who himself accompanied the expedition. Much of the scientific work was made possible by the gyro-stabilizer equipment of the yacht, which eliminated rolling.

DR. WALTER HOUGH, of the U. S. National Museum, who has been conducting field-work in southern Arizona for the past two months, was expected to return to Washington at the end of March.

F. TRUBEE DAVISON, president of the American Museum of Natural History and until recently Assistant Secretary of War for Aviation, and Mrs. Davison, will leave in June for a two months' exploration of the Kenya jungles of East Africa. They will join Mr. and Mrs. Martin Johnson on their trips, and will live at their home at Nairobi, on Lake Rudolf.

PROFESSOR H. J. MULLER, of the University of Texas, is spending the year abroad as fellow of the Guggenheim Foundation and has been working at the Kaiser Wilhelm Institute for the Study of the Brain at Berlin-Buch. He delivered a lecture at the institute on March 9. His subject was "Studies on Mutation."

THE subjects of the Dohme lectures to be given on April 20, 21 and 22 by Sir Henry Dale at The Johns Hopkins University under the general title of "Progress in Autopharmacology" are: "The Action of Histamine, and the Evidence Restricting its Probable Significance. Evidence for Other Natural Vasodilators." "Acetylcholine; its Natural Occurrence and Probable Function. Adenosine and its Derivatives." "Kallikrein. Vasodilators from Blood-vessels, Blood, Intestine and Brain. A General Survey of the Possible Functions of Such Constituents of the Tissues, in Relation to Those of Nerves and Specific Hormones."

PROFESSOR OTTO HAHN, director of the Kaiser Wilhelm Institute for Chemistry at Berlin, lectured on April 6 and 7, under the auspices of the Carnegie Institute of Technology at Pittsburgh on "Radioactive Elements and their Use in Chemical Research" and on "Gaseous Radio-elements and their Applications." Under the auspices of the Royal Canadian Institute, he lectured at Toronto on April 8, discussing in a popular manner progress "From the Ponderable to the Imponderable" in chemical work. Professor Hahn is the George Fisher Baker non-resident lecturer in chemistry at Cornell University during the spring semester, where he is lecturing on "Applied Radiochemistry."

PROFESSOR WILLIAM D. HARKINS, of the University of Chicago, gave on March 21 the last of a series of lectures on "The Neutron and Modern Alchemy." These lectures were presented at the University of Michigan, Princeton University, University of Kansas, University of Iowa, University of Chicago, Iowa State College, Kansas State College, University of Missouri, University of Indiana, and to sections of the American Chemical Society at Kansas City, Omaha, Chicago, Indiana, Wilmington, Philadelphia, and at the Research Laboratory of the Corning Glass Company.

THE Iowa Academy of Science will hold its forty-seventh annual meeting at Coe College, Cedar Rapids, Iowa, on April 21 and 22. Professor H. J. Gilkey, head of theoretical and applied mechanics at the Iowa State College, will be the speaker at that meeting. His subject will be "The Hoover Dam." Mr. Gilkey is a member of the board of consulting engineers on concrete problems for the Hoover Dam.

THE department of geology and geography of Northwestern University announces the institution of an exchange of lectures with Washington University, St. Louis. Under this arrangement Chester K. Wentworth, associate professor in the department of geology and geography, gave five lectures at Northwestern University, and Dr. Charles H. Behre, Jr., associate professor of economic geology, gave a similar series of lectures at Washington University. Professor Behre's lectures dealt principally with the geology of central Colorado and with various phases of economic geology, and those given by Professor Wentworth with problems of sedimentation and glaciation. Professor Wentworth also spoke before the Society of the Sigma

Xi of Northwestern University on "Power Sites in the Missouri River Basin."

UNDER a plan of exchange speakers the Iowa and Missouri Chapters of the Society of Sigma Xi have cooperated this winter. Professor A. G. Hogan, of the University of Missouri, addressed the Iowa Chapter at its first soirée on December 14, discussing "Nutritional Deficiency Diseases." On March 31, Dean George F. Kay, of the University of Iowa, addressed the Missouri Chapter on "Glaciation: the Background of Mississippi Valley Development." At the annual election of the Iowa Chapter of the Society of Sigma Xi new officers for the 1933–1934 period were named. Professor Henry A. Mattil succeeds Professor J. H. Bodine as president; Professor Norman C. Meier succeeds Professor John A. Eldridge as vice-president; Professor E. P. Tyndall succeeds Professor A. C. Tester as secretary; Professor Roscoe Woods completes a two-year term as treasurer. The society has sponsored a program of exchange speakers and soirées conducted by the College of Medicine, the College of Engineering, the department of mathematics, and the department of geology. Professor J Harlen Bretz addressed the society on February 15, on "The Grand Coulee," at the soirée conducted by the department of geology.

ANNOUNCEMENT is made that Sir Henry Dale, director of the National Institute for Medical Research of England and a leading authority on pharmacology, will be the principal speaker at the dedication of the new Merck Research Laboratory, to take place at Rahway, New Jersey, on April 25. Dr. Dale will be welcomed by the following guests of honor, who have accepted invitations to be present on the occasion of the formal dedication: Representing chemistry and the chemical industry, Lammot du Pont, president of E. I. du Pont de Nemours and Co., Inc., chairman of the Board of General Motors, Inc., president of the Manufacturing Chemists' Association; representing pharmacy and the pharmaceutical industry, J. K. Lilly, chairman of Eli Lilly and Company; representing medicine and the Public Health Service, Surgeon General Hugh S. Cumming, of the United States Public Health Service. The State of New Jersey will be represented by Governor A. Harry Moore. Over three hundred leaders in medicine and chemistry of the country will be present at the dedication.

The Johns Hopkins Alumni Magazine reports that a gift of $6,000 for the continuation of Dr. J. J. Abel's work on the pharmacology and chemistry of insulin has been received from the Commonwealth Fund.

Museum News states that the University of Richmond, Richmond, Virginia, will open a museum of biology in April. The museum will be housed in the new biology building of the university. It will occupy a room 34 by 40 feet with high windows so that 7-foot cases can stand beneath them, a room equipped with steel herbarium cases, and a room with 150 glass top insect cases in special cabinets and display cases. The case equipment is of bronze and plate glass by Russell. In addition, display cases will be set up in the halls and in each of the laboratories, and there will be a room with 24 cages for live animals, 22 aquaria of 12 to 200 gallons capacity, and a greenhouse on the roof. Other material besides the biological exhibits will also be shown in the museum. The building in which the museum is housed is three stories high, of brick and concrete, and includes lecture rooms, store rooms and the library of the department of biology with 500 feet of book-shelf space. Dr. John Wendell Bailey, professor of biology, is in charge.

DISCUSSION

NAMING THE GRAND CANYON

THERE seems to be some uncertainty in the public mind as to when the Grand Canyon was named and who named it. The first Spaniards to see it were members of a scouting party of the Coronado expedition in 1540 under Don Garcia Lopez de Cardenas, but, so far as the available records go, no name was then given to the great chasm. It was more than three centuries after this discovery by Cardenas that the canyon came into common knowledge though still with no name.

At last, in 1858, Lieutenant Ives and Professor Newberry wrote of it as the "Big Cañon" and the "Great Cañon." Then came Major Powell, in 1869, who was the first to explore its depths by descending through it in rowboats and he called it the "Grand Cañon."

In 1871 and 1872 Major Powell made a second descent of the Green and Colorado rivers. His report, published after this, spoke of it as the Grand Cañon (the Englished spelling "Canyon" was adopted later). The name was not recorded on any map till his topographers (of whom I was one) produced the first preliminary map of the region in the winter of 1872–73 in a tent in Southern Utah.

On Powell's first (1869) trip through the Grand Canyon it was not yet definitely named by him. John C. Sumner, one of his most active and intelligent men, says in a narrative he later wrote:

About fifteen miles below the Little Colorado the first bad rapid occurs in what I wanted to name Coronado Canyon. Major Powell told me it should bear my name if he got through and ever had the opportunity to place it on the Government map. Well, he got through all right, but forgot his vows and named it Grand Canyon.

There is no doubt whatever that the Grand Canyon was named by Major Powell not long after he came out of it on his 1869 trip.

FREDERICK S. DELLENBAUGH
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THE REACTION OF INDIVIDUAL BACTERIA TO IRRADIATION WITH ULTRA-VIOLET LIGHT

STUDENTS of the effect of ultra-violet light on bacteria have commonly accepted absence of multiplication, tested by clouding of broth or by colony formation, as the criterion of bactericidal action. This is a convenient but arbitrary endpoint, for "death" is not necessarily coincident with failure to multiply.

Observations on single organisms (*B. coli communis*) in the first hours after irradiation with measured monochromatic light, λ 2377 to 3022, show that there is a wide reaction zone between the behavior of cells which are not visibly affected and those which seem to be killed outright by the exposure. Single organisms which apparently have been unaffected grow and divide regularly on the surface of nutrient agar, producing typical colonies of normal-looking bacteria that ultimately coalesce into a confluent growth. Organisms which are exposed to quickly lethal doses show no increase in size and no cell division, but soon lose their high refractility, become beaded or irregularly refractile and degenerate into ghosts or shadows which are undoubtedly dead. In the intermediate zone between these two extremes the irradiated organisms present a remarkable picture. They increase in size, especially in length, without apparent inhibition, but do not divide when the normal adult stage is reached; so that long filaments of clear, highly refractile protoplasm are formed that look like spaghetti and may attain measured lengths of 50 to 150 microns. The transverse diameter is 1 to 3 times that of normal bacteria. These long forms are actively motile in a fluid medium, progressing with a sinuous motion, or drilling back and forth like spirochaetes. In 2 to 4 hours at 37.5° C the cells reach a limit of individual growth, forming long rafts of parallel filaments on an agar surface. Then cell division or degenerative changes begin. (1) The long cells may undergo a gradual degeneration, with loss of hyaline refractility, beading and ghost formation. (2) They may divide by cross fission into a number of large or small units, which then degenerate without further growth. (3) One or more normal-looking daughter cells may pinch off at one end and multiply rapidly to colony formation. The rest of the filament follows courses 1 or 2 above. Daughter cells have not been observed to form filaments; the reaction is limited to cells directly exposed to ultra-violet light.

Here is an apparent separation of two coordinate functions commonly essential to life. Cell division is regulated by a mechanism which is much more sensitive to wave-lengths below 3,000 than is the concomitant function of growth. By appropriate exposures the one can be temporarily or permanently suppressed, while the other proceeds for hours without hindrance. Ultimately, however, a limit is reached, and unless the division mechanism is restored the cell degenerates. A further study of this cell reaction is in progress.

FREDERICK L. GATES
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LACTOBACILLI IN FROZEN PACK PEAS

MICROBIOLOGICAL analyses of frozen pack peas held in storage from 1 to 26 months show that lactic acid bacteria tolerate well cold storage temperatures. The peas under consideration, representing several batches, were packed in a variety of ways—blanched and unblanched, dry and in brine, in 1 pound paper containers, and in No. 2 tin cans both vacuumized and unvacuumized. The storage temperature in most cases was 15° F., though some containers of the current year's pack were held at minus 5° F.

Some 40 samples from the different packs have been studied from the spoilage view-point alone, the containers being held at ordinary room temperature for 2 to 7 days, at the end of which time the contents were analyzed bacteriologically. Without exception the peas presented a bleached appearance and had a sour odor. Those in tin had developed more or less pressure in the container. The clouded liquors gave positive ferric chloride tests for lactic acid. Acidities of 1.0 to 1.3 per cent., calculated as lactic acid, were found, and the pH values ranged between 4.2 and 4.6. Direct microscopic examination invariably showed gram-positive rods, and medium-sized gram-negative rods were often present, but in smaller numbers. Appropriate cultural technique yielded the *Aerobacter aerogenes* ("colon") type and lactobacilli.

Study of 15 cultures of the lactobacilli isolated places the majority with the *Lactobacillus cucumeris* type, since they ferment arabinose and trehalose in addition to sucrose, dextrose and maltose. Inoculation of pure cultures of these into sterile peas yielded products entirely similar to the fermented frozen pack peas as regarded appearance, sauerkraut-like odor and acidity.

Organisms of this type are widely distributed and occur commonly on vegetables, and their presence on shelled peas is to be expected. Since they are not spore formers, it is noteworthy that they withstand a temperature of 15° F. for over 2 years. While no peas stored at minus 5° F. for more than a month have to date been analyzed, the lactobacilli in all probability would persist at this temperature, for experience has shown that micro-organisms generally tolerate zero Fahrenheit as well as or better than higher freezing temperatures.

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VITAMIN A IN THE PIMIENTO PEPPER

THE pimiento pepper is utilized in the culinary art chiefly as a condiment. This rather limited use in the diet may perhaps be an explanation for the relative paucity of research concerning its food value.

MacLeod and Booher[1] report that the vitamin C content of the canned product is equivalent to that of fresh grapefruit, and analyses have determined its chemical composition.[2] Capsanthin, or an allied pigment so masks any other color present in the pepper that it seems expedient at this time to call attention to its high carotene content.

In the biological study the Sherman technique was followed. Rats varying from 40 to 50 grams at weaning were put upon his vitamin A-free diet until symptoms of depletion were manifest, *i.e.*, signs of ophthalmia and stationary or slightly declining weight. Fresh, commercially canned and dried pepper were each then fed upon three different levels with an average of eighteen animals in the several groups.

This preliminary investigation has shown that, computed upon the dry basis, four milligrams of pepper induced a growth response above that of the Sherman unit. Further, a chemical assay has indicated from 200 to 300 mg of carotene per kilogram of the dried material. Work is in progress to establish the unit level for the pimiento pepper and to ascertain its carotene value.

LEAH ASCHAM
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EARTHQUAKES IN THE HOLY LAND: A CORRECTION

IN an article on earthquakes in the Holy Land[1] there is given a list of 207 shocks, of which there is record, between the years 1606 B. C. and 1927 A. D. Among these are 27 dates from an Arabian authority, As-Soyuti, whose work appears in translation in the *Journal of the Asiatic Society of Bengal.* In transcribing these dates I failed to observe that they were stated as A. H., *i.e., Anno Hejira,* instead of *Anno Domini.* They are, therefore, as quoted in my list something over six centuries too early. The corrected dates are as follows:

A.H.	A.D.	A.H.	A.D.	A.H.	A.D.
94	712	434	1042	552	1157
98	716	455	1063	565	1169
130	747	460	1067	575	1179
220	835	462	1069	578	1182
233	847	479	1086	597	1200
242	856	484	1091	600	1203
245	859	532	1137	702	1302
393	1002	538	1143	791	1388
425	1033	551	1156	889	1484

Inasmuch as the Hejira dates from July, 622 A. D., and there are adjustments of the calendar dates for fractions, these figures may be off one year. It is also probable that the original dates are approximate. Hence where As-Soyuti differs by a year from others given in the list as published, one shock only is presumably meant.

In this connection I would call attention to a very ancient, yet definite observation regarding the now well-known earthquake fault that traverses the eastern slope of the Mount of Olives:

> And His feet shall stand upon the Mount of Olives which in that day is before Jerusalem on the east; and the Mount of Olives shall be cleft in the midst thereof toward the east and toward the west, and there shall be a very great valley; and half of the mountain shall remove toward the north and half of it toward the south.[2]

Activity on this fault was the occasion of destructive tremors in 1927. That it was the scene of more obvious displacement some 2,500 years earlier we can not doubt in view of the graphic description of the Old Testament writer, although his identity and the exact date of his prophecy are matters of uncertainty, especially with reference to this particular passage.

BAILEY WILLIS
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A RARE PUBLICATION

THE library of the Academy of Natural Sciences of Philadelphia recently acquired from a dealer in second-hand books a copy of Volume 1, 1892–94 (1895) of the Transactions of the Natural History Society of Queensland. The fact that this is not listed in the Union List of Serials and that the natural

[1] Grace MacLeod and Lela Booher, "The Antiscorbutic Vitamin Content of Some Preserved Foods," *Jour. Home Econ.*, 22: 588. 1930.
[2] J. G. Woodroof and J. E. Bailey, "Pimiento Peppers," *Ga. Exp. Sta. Bull.*, 150. 1929.
[1] *Bull. Seismol. Soc. Amer.*, Vol 18, 1928.
[2] The Old Testament, Zechariah, 14, 4–6, 520 B. C.

history library of the British Museum and that of the Linnean Society of London, when their library catalogues were issued a few years back did not have this volume, would indicate that this is a rare work. The library of the Zoological Society of London lists the work in its catalogue, although the new species described in the "Transactions" are not noted in the *Zoological Record*. There are but two articles of taxonomic interest in the volume, one by C. J. Wild, describing *Dendroceros subtropicus*, a new species of moss (pp. 49–50); and "Australian Lepidoptera: Thirty New Species," by Thomas P. Lucas, entirely moths (pp. 101–116).

WM. J. FOX

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

SCIENTIFIC APPARATUS AND LABORATORY METHODS

DOUBLE STAINING BY THE CAJAL–BROŽEK METHOD

WHEN making durable microscopic slides, many different ways of dyeing are in use, of which some are of chief importance in cytological work. But the double staining is such that only quite definite parts of the cells, *e.g.*, chromosomes or nucleoles are stained by one color; whereas the other color stains another part, *e.g.*, cytoplasm. The method, which the author of this account wishes to describe briefly, has been worked out with a few alterations, by Ramon y Cajal. It has been used from time to time and still is used in zoological work, but only for the last three years in botanical studies, where it has been introduced by Professor Arthur Brožek with considerable success, in a modified form.

It is a cytological method, which stains the chromosomes splendidly, and when compared with Heidenhain's iron-hematoxylin method, so much used nowadays, has certain advantages, which will be pointed out below. By our method, the chromosomes are stained a bright red with magenta—which is basic fuchsin, while the nucleole and the other parts are stained sky-blue with picro-indigo-carmine. Picro-indigo-carmine is a mixture of 2 parts saturated aqueous solution of indigo-carmine and one part of picric acid. Both the magenta and picro-indigo-carmine are used in concentrated solutions.

The process of staining is quite simple and short. The preparation—paraffin sections mounted on supporting glass—is freed from paraffin by xylol and brought through a mixture of alcohol-xylol and then through a series of alcohol baths to water; after which the object is placed for 3 to 5 minutes in concentrated magenta. The exact time must be found out for various objects, although the preparation always appears quite right, if treated for the length of time mentioned above. At the expiration of this time, the object is rinsed in distilled water and immediately placed in picro-indigo-carmine, where it is left for 10 to 15 minutes. It is better to find out the precise time for various objects by experiment. Then the object is placed in very slightly acid water for about one minute, into a beaker of distilled water with 1 to 2 minims acetic acid, after which an automatic differentiation is made with 80 per cent. alcohol. The slip is simply rinsed from the acid water, allowed to drain and plunged directly into 80 per cent. alcohol, where it remains until the red stain from the section remains, which can easily be seen with the naked eye. The time for differentiation is one half to one minute. Then the object is brought into 96 per cent. alcohol, 100 per cent. alcohol and a mixture of alcohol-xylol to xylol, and then mounted in Canada balsam. The whole process, from the placing in xylol to the mounting in Canada balsam, does not last more than 40 to 45 minutes. This is a great advantage when compared with Heidenhain's method, in which the procedure in the best case lasts for eight hours, and with careful working may even last for two and a half days. Another advantage is that the chromosomes stained with magenta can be distinguished from one another, even when they lie close to each other, or actually on each other. Such a method with hematoxylin is quite impossible on account of the covering color. A certain disadvantage of the method is that it can only be used successfully after sufficient acid fixing. It is best to use Nawashin's method, which is at the same time the most perfect, after fixing—15 parts 1 per cent. chromic acid, four parts formol and 1.5 parts glacial acetic acid. This method has been tried with success after fixing in highly diluted HNO_3. After using Němec fixing solution I., color tends to disappear—100 ccm 1 per cent. chromic acid and 8 ccm formol—but no change is apparent after using Němec II—50 ccm 1 per cent. chromic acid, 50 ccm 3 per cent. bichromate of potash, and 8 ccm formol; or after Regaud's method—80 parts 3 per cent. bichromate of potash and 20 parts formol. It proceeds badly after alcohol or formol fixing, but quite well after sublimate with acetic acid. There is no need to mention other fixing methods, which are for the greater part special. It is important that our method works excellently after Nawashin's fixing, which is most used to-day in plant-cytology. This method can be recommended, as it furnishes beautiful slides in a relatively short time,

which can be studied in all their details, drawn and excellently photographed with a green filter.

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A NEW PARAFFIN EMBEDDING MIXTURE

DIFFICULTY in obtaining a paraffin of satisfactory consistency in which to embed and section biological material led to the development of the following formula. The use of neither of the substances added to the common paraffin is original with the writer (having been in fairly common use fifteen to twenty years ago) although the proportions and combinations may be.

A stock solution of crude rubber in paraffin is first made. Crude rubber is available in thin sheets, either smoked or unsmoked. Both kinds work equally well. The sheets of crude rubber are chopped up with a scissors and dropped in melted Parawax or any similar common paraffin. The paraffin should be smoking hot and the mixture should be stirred occasionally. Three to four hours are required to completely melt the rubber. About 20 grams of rubber can be dissolved in 100 grams of paraffin.

EMBEDDING MIXTURE

Parawax	100 grams
Rubber-paraffin mixture	4–5 grams
Beeswax	1 gram

Filter through paper (paper towels serve this purpose excellently).

This mixture is pale yellow in color, does not crystallize readily, and is of a waxy consistency that sections unusually well. It has been in use in this laboratory for the past three years and has materially increased the success of large classes in micrology.

ROBERT T. HANCE
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SPECIAL ARTICLES

PHOTOPERIODISM AS A CAUSE OF THE REST PERIOD IN STRAWBERRIES

EXPERIMENTS made by the authors on the light requirements of strawberries have suggested an explanation of the cause of the rest period in this plant. The strawberry plant in northeastern United States becomes more or less dormant with the onset of low temperatures in the fall. Low temperatures alone are not responsible for dormancy, for many varieties remain dormant even when taken into a warm greenhouse in late fall. However, if plants are first given a low temperature rest period until about January 1, when brought into a warm greenhouse, all varieties grow vigorously, even though the days are still short. After the rest period has been broken, leaf growth and flower production is rapid. Plants covered with boxes in the field in January, so placed as to exclude all light, blossomed at about the same time as those in the open. Temperature and not light was the controlling factor after the rest period was broken.

If plants in the greenhouse are given additional light for several hours each night, beginning in November before they have had a low-temperature rest period, many but not all varieties grow vigorously. Each variety and species shows a characteristic response to the length of the daily light period at this time. If, however, the daily light period is lengthened beginning in early fall (September 1) before the days become short, the plants of all varieties we have tested make a vigorous vegetative growth throughout the entire winter and do not require a low-temperature rest period. Some varieties under such increased light periods produce some flower clusters, depending on their particular daily light period response. In contrast, plants not given additional light start fruit bud formation with the onset of short days, and if the temperature is above 60° F., plants of many kinds go into the rest period.

In one test, plants of some 51 varieties were given electric light until 10:00 each night to supplement daylight, beginning September 1. Until February their growth was almost entirely vegetative. They produced an average of 0.7 flower clusters per plant by that time, while control plants under normal light but similar temperatures had averaged 3.8 clusters. By June 9, the control plants had averaged 20.2 clusters, while the plants under the lights had averaged 4.8 clusters each. Continued growth seems to be correlated with relatively constant exposure to the photoperiodic requirements of the particular variety. Growth in some form apparently can continue over a fairly wide range of light exposures. For ordinary varieties very short daily light periods initiate a rest period, short light periods result in continuous fruiting and longer periods in vegetative growth only. Rest periods then seem to result from nutritional conditions following exposures to short daily light periods. *Fragaria virginiana* and varieties adapted to the Northern and Eastern states require a rest period after exposure to days as short as 12 hours daily light. *F. chiloensis,* varieties of the Northwest derived in part from it recently, and Southern varieties do not need a rest period until the days are much less than 12 hours. In fact, after exposure to days as short as 9 hours at the relatively low temperatures used they still grew freely.

Strawberries are grown commercially from regions having several weeks of continuous light in midsummer, such as Alaska, to regions of successively shorter daylight periods southward to Ecuador having 12 hours of daylight, and in winter, in Florida in days with less than 11 hours of daylight. Varieties that succeed in subtropical climates do not require a rest period, at least when grown under the day lengths in the subtropics. Although there is evidence that better growth may sometimes be obtained on such varieties after exposure to low temperatures for a time, they do not require a rest period under light conditions that prevail at Washington, D. C.

A study of the responses to light and rest periods seems to show that the strawberry varieties now grown in regions of short and of long days have actually though unconsciously been selected for adaptation to the light and rest period conditions of those regions. Varieties grown in subtropical regions are sufficiently adaptable to grow continuously under light periods of less than eleven to more than thirteen hours, and in these regions do not require a dormant period. Varieties adapted to Northern United States do not grow well under the short days of Southern states, and soon die out, while southern varieties make too vigorous a growth under the long days of summer in the North.

The strawberry is an evergreen, and upon exposure to suitable light periods some varieties after becoming dormant resume growth. In others apparently nutritional conditions have progressed to a point where light alone as we have used it is insufficient to cause renewal of growth. If the rest period in some other plants, especially in the deciduous ones, is due to similar causes, attempts to produce continued growth or to break the rest period by the use of lights should start before the leaves have fallen, or in some cases even as early as the end of June when the days are longest or even earlier to prove successful

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A "SCURVY-LIKE" DISEASE IN CHICKS

IN the course of nutritional studies we have observed the development of a scurvy-like disease in baby chicks. The first external symptoms are usually observed about the third week. They are nervousness and lameness, which is apparently due to stiffness of the hock joints. These symptoms are followed by bleeding from pin feathers on the neck, wings or thighs. About the same time blood clots appear beneath the skin and in the muscle of the thigh, around the hock joint and at the base of the wings. Closely following these abnormalities are frequent hemorrhages around the head, neck, back, ribs, breast and keel. Abdominal hemorrhages and small hemorrhagic spots in the muscle of the intestinal wall are common. The most frequent symptom is that of dark erosion spots on the gizzard lining[1] similar to the condition described by McFarlane, *et al.*[2] In these experiments approximately 95 per cent. of the chicks had erosions on the gizzard lining, while over 70 per cent. were affected with external or internal hemorrhages. In chicks suffering from the disease the bones were often brittle and the bone marrow dry and colorless. The blood showed an extremely low hemoglobin content (as low as one gram per hundred cubic centimeters of blood) and the blood picture microscopically resembled that described by Hess[3] for human scurvy. There further seemed to be an association between the symptoms described and growth in that the more rapid the growth the earlier the symptoms occurred and the more severe they were.

The symptoms described were produced by feeding a ration composed of fish meal, ground yellow corn, yeast, ground oyster shell and sardine oil or cod-liver oil to chicks confined in battery brooders. Rations in which the yeast was omitted produced the symptoms to a lesser extent. The substitution of 10 per cent. of dried skim-milk for the yeast and a portion of the fish meal produced about normal chicks. However, a few cases of eroded gizzard lining and small hemorrhages were observed, even when the milk was included in the diet. Five grams of cabbage per bird fed to affected individuals during the fifth and sixth weeks resulted in a complete recovery at the end of this period.

In view of the symptoms described, the diets used in order to produce the disease and the fact that cabbage in small quantities brought about a cure, we believe that, contrary to previous reports, growing chicks may suffer from scurvy due to an absence of vitamin C in the diet. We further believe that they are either unable to synthesize vitamin C or under certain conditions they are unable to synthesize this vitamin in sufficient amounts to meet their requirements.

A more detailed report of these investigations is being prepared for publication.

WALTHER F. HOLST
EVERETT R. HALBROOK

UNIVERSITY OF CALIFORNIA

[1] Similar gizzard affections have been observed in baby chicks as early as the seventeenth day of incubation.

[2] W. D. McFarlane, W. R. Graham, Jr., and G. E. Hall, *The Journal of Nutrition*, 1931, Vol. IV, 331.

[3] Alfred F. Hess, "Scurvy, Past and Present," Philadelphia, 1920.